

and various points of view are necessary for a complete examination.

On comparing this photograph with the well-known drawings of the same object by Lord Rosse, Bond, Struve, Lassell, Trouvelot, Secchi, and others, it will be noticed that most of the small stars, about which there is no dispute, are represented, and the outlying streamers are well indicated. It is worthy of remark also that in this photograph with an exposure of only 137 minutes I have depicted stars almost the *minimum visible* in this telescope, and it is not unreasonable to hope that by still further prolonging the exposure and by still further study of photographic processes, stars and details entirely invisible to the eye may be secured. With this object in view, I have under consideration a new form of mounting which will permit of continuous exposures of six hours.

During the month of March 1882 I also made four photographs of the spectrum of the nebula in *Orion*, which are described in the May number of the "American Journal of Science." Two of these were obtained with the slit spectroscope I usually employ for photographing spectra of the stars, and they show two lines in the ultra-violet plainly, besides the traces of two others. The first-mentioned two are hydrogen  $\gamma \lambda 434\text{o}$  and hydrogen  $\delta \lambda 410\text{r}$ ; the others are too faint to give a good estimate of the wave-length.

The other spectrum photographs, taken without a slit, show that two of the condensed masses preceding the trapezium give a continuous spectrum, and therefore contain either gas under pressure, or liquid or solid matter.

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*Some Remarks on Mr. Newcomb's Paper "On the Instructions for Observing the Transit of Venus formulated by the Paris International Conference."* By E. J. Stone, Esq., M.A., F.R.S.

It is greatly to be regretted that the step which was taken by the English Committee in the October of 1881 of issuing a draft of Instructions to Observers for consideration and discussion did not lead to any direct interchange of ideas between the American and European astronomers. I believe that an interchange of ideas, such as that contemplated, would have led to the adoption of "Instructions" which were identical in all essential points, and I hope that this object may yet be secured. The remarks made in the Notices for April by Mr. Newcomb, on the Instructions issued by the International Committee on the Transit of *Venus*, are interesting, and require consideration; but unfortunately they are stated to be simply "an individual contribution to the discussion," and the time for discussion is

fast passing away and that for action is approaching. It appears to me, however, that there are really but few important points upon which Mr. Newcomb's views differ essentially from those which were attempted to be embodied in the International Instructions, and that something like common action is still possible. All astronomers have their favourite way of describing the "real contact," but mutual concessions on unimportant points are necessary if combined action is desired. I have, in this note, thrown together a few remarks which have occurred to me whilst reading Mr. Newcomb's paper. I hope these remarks will, at least, be considered before any Instructions are issued to the American observers which may prevent their observations of contacts from being ultimately combined in one general discussion with those observed by the French, German, and English observers. A chain is not stronger than its weakest link, and the value of the large number of observations of contact at "Ingress retarded" and "Egress accelerated," which will undoubtedly be secured, if the weather is favourable, by the American observers, will depend very much indeed upon the number of contacts of "Ingress accelerated" and "Egress retarded" which may be available for direct comparison with them, and for most of these the American astronomers will be dependent upon the observers of other nations.

The most important point upon which Mr. Newcomb's views appear to differ from mine is the question of the brightness of the field of view in which the contacts should be observed. Mr. Newcomb considers "that the observer should have the solar disk as bright as the eye could bear with entire ease and comfort." I cannot think this desirable. If the sky was covered with thin cloud or haze at one of the stations, the difference would be excessive between the brightness of the field available at that station and the brightness which would be adopted at stations where the sky was perfectly clear. There are also very considerable differences in the degree of illumination of the field of view which different observers can bear with ease and comfort, and all the phenomena of irradiation are presented in an aggravated form when the brightness of the field is excessive. The complications introduced by the phenomena of irradiation are perhaps the difficulties chiefly to be feared in attempts to determine the value of the solar parallax from a discussion of internal contacts. I think, therefore, that we are not justified in adopting a brightness of the field of view which must increase to the utmost these difficulties.

But the changes in the illumination of the Sun's limb, which take place near the point of contact, can only be recognised by contrast; it is therefore essential that the brightness of the field should be sufficient to allow these differences of illumination to be readily distinguished, and the adoption of a field of view with a very feeble illumination is not, therefore, desirable. It will be found, on consideration, that difficulties quite as great as those

dependent on the irradiation phenomena can be introduced by unduly diminishing the brightness of the field. Pairs of very fine spider webs at distances corresponding to a second of arc in the focus of a positive eyepiece appear to afford a delicate test of a suitable field. But this test requires great care in its application, and I am inclined to think that a rougher but better practical test is afforded as follows:—

Let an observer mark the part of his wedge where he can just observe the Sun's limb in a perfectly clear sky without discomfort. This degree of brightness is, I consider, excessive. Let him also mark the part of the wedge at which, under the same circumstances, he could just distinctly and clearly observe the Sun's limb. This field is the faintest which that observer could adopt, and is, in my opinion, decidedly *too* faint. If the observer adopts the mean portion of the wedge as giving, under a clear sky, a standard brightness of the field of view, he cannot err much either in excess or defect. There will of course be slight differences of opinion amongst observers of what constitutes the *best* degree of illumination of the field, but all the skilled observers who have examined the point at Oxford agree that the field of view thus found is an agreeable one to observe in, and that the brightness is sufficient to allow the rice-grains and the minute details of solar spots to be well seen. Slight changes in the portion of the wedge recommended for use are unimportant, but it is desirable that excessive differences in the brightness should be avoided, and that some approximation to uniformity in this respect should be secured. The brightness of the field fixed as described, on a clear day, would have to be learnt by practice as a habit of the eye, and adopted, as closely as possible, by shifting the wedge to meet the varying conditions of the atmosphere at the time of observation. This is the recommendation which I should make with regard to the brightness of the field of view in which the contact should be observed. It secures approximate uniformity, and it excludes extremes of either excessive brightness or undue faintness in the illumination of the field of view.

I could not join in Mr. Newcomb's recommendation of the use of Dawes' solar eyepiece. This eyepiece is valuable for the examination of detached portions of the solar disk; but the field of view is exceedingly limited, and when clouds were passing there would be practical difficulties in keeping the point of contact exactly in the centre of the field, whilst the effects of the stop, near the edge of the field, would be most injurious in the contact observations.

I must confess that I cannot understand the difficulties which Mr. Newcomb experiences in forming a precise conception of what should be understood by the phrase "illumination of the apparent limb of the Sun" and "how discontinuity in that illumination should be noted." The "illumination of the *apparent* limb of the Sun" means the illumination of the limb of the Sun

as seen—the visible limb; and the discontinuity of the illumination can be noted as follows:—Suppose *a*, *c*, *b* to be three small portions of the visible, or apparent, limb of the Sun, *c* being the portion near which the internal contact takes place. Before contact at Egress, or after contact at Ingress, there will be no sensible difference between the illumination of the portions *a*, *c*, *b*, but near the contact the illumination of the Sun's limb at *c* will be less than the illumination either at *a* or *b*. This difference between the illumination at *c* and at *a* and *b* constitutes a discontinuity in the illumination of the Sun's limb near the point of contact, which every observer must see, if the sky is clear, near the internal contacts. The observers are therefore simply asked to give at Ingress the last time at which they are *certain* of the existence of such a discontinuity as independent of mere atmospheric tremor; and at Egress, the first time at which they are certain of such a discontinuity. In the English Instructions the observers will be asked to give also the times of deepest shade, when the black drop, ligament, haze, or shadow ceases to be as dark as the outer edge of the planet *Venus* at Ingress, or first becomes as dark as the outer edge of that planet at Egress. I hope and believe that the observers will experience no difficulty in understanding these instructions, and that they will observe the contacts as defined.

The contacts as defined in the International Instructions are the same as those which Mr. Newcomb would, *apparently*, wish to have observed. I say *apparently*, for in many parts of Mr. Newcomb's paper it would appear that he prefers to leave the observer without any precise definition of the contacts to which his attention should be directed—a course of proceeding which, in my opinion, could lead to nothing but a complete failure: but if Mr. Newcomb simply recommends the observers to give at Ingress “the time when light is about to glimmer all the way across the dark space between cusps,” the recommendation would do no harm. The time which should then be given by the observers would be the time when there was last seen a distinct and persistent discontinuity in the illumination of the Sun's limb at the point of contact. The chief difference between the definitions appears to be that Mr. Newcomb prefers to direct the attention of the observer to the light of the cusps which is encroaching upon the “*dark space*” between them, whilst in the definition adopted by the International Conference the attention of the observer is directed to the disappearance of the *dark space* between the cusps.

But if Mr. Newcomb's definition were adopted it would be necessary to introduce an exception, and to caution the observers from taking the time when the light of the aureole, penumbra, or “sunlight through the atmosphere of *Venus*” began to glimmer across the dark space between the cusps. Without this caution the observed contacts would be very uncertain and the times recorded would be earlier at Ingress than those which Mr. New-

comb requires by about a minute of time. Mr. Newcomb's definition and that of which it is a modification and improvement, given in the *Notices* for March 1877—viz. "the time at which true sunlight is first seen all the way around the following limb of the planet"—is open to the objection that it is of a negative character. The first time at which bright sunlight is seen may be caught up, through clouds, long after all touch between the limbs has ceased. Such observations when recorded as contacts destroy the value of a large number of good observations, unless rejected, and this is always an unsatisfactory proceeding, and one sometimes open to misconceptions. If properly understood and strictly followed, the definition of Mr. Newcomb should lead to identical results with that adopted at the Paris Conference; but I cannot regard it as an improvement. To the recommendations to leave each observer from a knowledge of the general theory of the subject to observe any contact which he considers best under existing conditions, and to the proposals of IV., page 280, I object *in toto*.

I have no wish to indulge in mere verbal criticisms, but it is desirable to point out that Mr. Newcomb's views of the nature of the internal contacts have been very much based on model-practice, and that, without great care, this model-practice is misleading. In the case of the model we have the Sun's limb represented by one hard edge and the planet represented by a disk. There are therefore complicated phenomena of diffraction beyond the hard edge which represents the Sun's limb, and of interference between the limbs of *Venus* and the Sun, which vary as the contact approaches, and which have nothing strictly analogous to them in the case of the actual transit; whilst, on the other hand, the light which is refracted through the atmosphere of *Venus* in the real transit introduces complications to which there is nothing strictly analogous in the models in general use. Whilst therefore model-practice may be useful to observers, in preparing them for the slowness with which the contacts are established and in exhibiting changes somewhat similar in general character to those which will be presented in the real transits, the observers require to be carefully cautioned against expecting to see the *same* succession of phases identically reproduced in their model-practice and in the real transit. And here I must point out that when Mr. Newcomb speaks of the "true internal contact" and of the "sharp cusps of *Venus*, instead of appearing sharp as in *their true form*, appear rounded at their termini by the black drop or other forms of distortion," the language used is misleading. There is no "true internal contact," neither are there cusps, except as we see them projected on the Sun's disk; and the phenomena as seen are the *real phenomena*. No doubt, if there was no such thing as the diffraction of light, if the telescopic image of a bright point was a point, if there was no dispersion of light through the atmosphere of *Venus*, then the definition of the true internal contact would be easy, and the

sharp cusps of *Venus* might then be seen without any blunting, rounding, or diffusion of the cusps. But we must accept the laws of nature as we find them, and no good, but much harm, will result from our ignoring the existence of causes which prevent the appearances which would be found to exist under the supposed laws of formal *geometrical optics* from being realised in the actual transit. We have already suffered too much from this error.

I am unable to follow Mr. Newcomb's remarks respecting Mr. Tebbutt's observations in 1874. There is no doubt that Mr. Tebbutt's "contact" at Ingress was observed rather later than the general "average contact," but there are later times still given for contact; and the error of Mr. Tebbutt's observation, as measured by the difference between the angular separation of the centres of *Venus* and the Sun at the time given and the mean angular separation, is only about two-tenths of a second of arc. The observation is therefore neither worthless nor very wild. At Egress the time given by Mr. Tebbutt corresponds to an angular separation of the centres which is nearly the mean angular separation of all the observers.

In conclusion, I would remark that if some approximation to uniformity in the instrumental equipments of the observers be secured—if good telescopes of nearly the same, and not too small, apertures be employed; if magnifying powers of not much less than 150 be used; if the observations of contact are made in fields of view of which the degree of illumination is not greatly in excess or defect; and if, above all, the attention of the observers is directed to a contact which is distinctive enough to be recognised: then, in my opinion, we may be certain of a substantial success, unless clouds should intervene and spoil the observations at several of the most important stations. To diminish this risk as much as possible, we have increased the number of stations; more we cannot do to insure and deserve success. But, whilst I feel confident of success if all the observers attempt to observe the same thing under somewhat similar conditions, if each observer is to observe any kind of contact which appears good in his own estimation, without reference to what is being done by other observers, then there must be as many values of the solar parallax deducible from attempts to combine these discordant observations as there are different kinds of contact observed at the opposed stations of accelerated and retarded effects of parallax.

In the paper of March 1877, *Monthly Notices* (vol. xxxvii.), to which Mr. Newcomb refers as explaining his views of the internal contact, the attention of the observers is directed to observing the first time at Ingress and last time at Egress when direct sunlight is seen all around *Venus*. Now, if such a definition should be adopted by the American observers, whilst the observers at the Cape, Madagascar, Australia, and New Zealand record the times when they last see "the illumination of the

Sun's limb near the point of contact as dark, or nearly as dark, as the outer edge of the planet," then an attempt to obtain a value of the Sun's parallax from a discussion of these observations, on the assumption that the contacts observed refer to the same angular separation of the limbs, will most certainly lead to a value of the solar parallax which is in excess of the true value. If the contacts observed were reversed at the different stations, then the result that would be obtained would be too small. I call attention to the point now, before the transit has taken place, in order, if possible, to prevent the serious confusion and doubt which may arise if any large number of observers are directed to observe a contact of an essentially different character from that which will be observed by the general body of the observers. If this caution is not attended to, it will be possible to obtain different values of the solar parallax by different combinations of the contacts; but of course no one entrusted with the calculations should attempt to combine in one discussion discordant material. It would, however, be far more satisfactory if the collection of this discordant material could be prevented rather than it should be rejected after the transit as worthless for the objects in view.

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*Curves showing the Changes in the Adopted Diameter of the Moon  
as given by the Observations in the Greenwich Lunar Reductions 1750-1830.* By E. J. Stone, M.A., F.R.S.

The results upon which the curves are based have been extracted from "The Greenwich Lunar Reductions," vol. ii., Section iii., Comparison of Moon's Observed and Tabular Place, pages [1] to [293]. It is from a discussion of the results given in this section that the coefficients of the parallactic inequality were deduced by Sir G. B. Airy, which show an inequality with a period of about forty-six years. The observations have been divided into groups extending over periods of about nine years, identical with those adopted by Sir G. B. Airy. If  $E_1$  and  $E_2$  denote the excess of observed longitudes over tabular longitudes as deduced respectively from the observations of the first and second limbs, then  $E_1$  and  $E_2$  have been extracted directly from Section iii. for all the days on which both limbs were observed in Right Ascension and a North Polar Distance is available for the deduction of the observed longitude. The mean results are given in the following table. The results, divided by 2, will be found essentially the same as those which I have already given in the *Notices* for Dec. 1881, but in the former paper the results for the last year of each period were not included. In the present table the results of the last year of each group are included. The general form of the curve is not, however, altered by this difference in the grouping:—